

*Reply to Professor Newcomb's Two Questions.* By E. J. Stone,  
M.A., F.R.S., Radcliffe Observer.

Before answering Professor Newcomb's two questions, I feel it necessary to call attention to his statement that in the *Monthly Notices* for March he applied my theory to the special case of the transit of the Moon at Oxford on 1892 January 6, and "without neglecting any quantities in my theory" was led to an inconsistency. Now, this statement is certainly inaccurate. It is simply because Professor Newcomb has neglected certain quantities which, if my views of the physical bases of our time-measures are sound, cannot be neglected without error, that the inconsistency arises; and in my reply I called Professor Newcomb's attention to this point as the real one at issue, but apparently without success.

I now come to Professor Newcomb's questions.

1. "What would have been the tabular error of Hansen's tables of the Moon given by the transit at Oxford on 1892 January 6 if Carlini's tables of the Sun had been continued in use in the *Nautical Almanac* until the present time?"

I understand by this question that the data given in the *Nautical Almanac* are supposed to have been continuously used for the computation of our "sidereal times at mean noon," and in the actual determinations of our local sidereal times, and not merely printed without being used. In this case the answer is that the R.A. from Hansen's tables minus observed R.A. on 1892 January 6 would have been  $= -0^s.10$ , the result which is given in the *Monthly Notices* for November, and is quoted by Professor Newcomb under the form "Hansen minus observed, corrected." I have published the answers to *this and similar questions for all the observations of the Moon made at Oxford since 1880.*

2. The answer to the second question is that the effects produced on  $-0^s.10$ , in the supposed case, would be very small; for they would merely be those due to the difference between the "local sidereal time," which would, if no change had been made, have been accepted by astronomers as 1892 January 1, subject, of course, to any errors of that assumption, and that which would have been accepted as the epoch 1892 January 1, the instant of departure for the new system of time-measures, together with the difference between the sidereal equivalents for the two different intervals of absolute time used as "unit days" before and after the change, multiplied by the number of "unit days" which had elapsed since the change was made, which in the cases supposed would be equal

$$5 \times \left[ 86400^s \left( \frac{N + \delta N}{N_0} + \frac{N + \delta N}{2\pi} \right) - 86400^s \left( \frac{N}{N_0} + \frac{N}{2\pi} \right) \right]$$

$$= 5 \times 86400 \frac{dN}{N_0} \left( 1 + \frac{N_0}{2\pi} \right)$$

where  $dN$  is the difference between the values of the Sun's mean motion in longitude used in the construction of Le Verrier's and Carlini's solar tables,  $N_0$  the Sun's mean motion in the "physical" as distinguished from the "unit day." In the computation of the small corrections it is sufficient to replace  $N_0$  by  $N$  or  $N + \delta N$ . The method is, I hope, here explained in sufficient detail for anyone to follow; it is applicable to all such cases, and it is the one which I have used in allowing for the changes in the scales of time-measurement.

Having given distinct answers to Professor Newcomb's two questions, I may perhaps be allowed to explain that the corrections which I have applied in passing from a system of time-measures based on the use of one set of solar tables to those based on the use of a different set of solar tables, are strictly deduced from, and are the necessary mathematical consequences of, the physical bases upon which our measures of time rest. It is the use of two independent systems of time-measures, one based on the geocentric motion of the Sun, the other on the motion of the meridians in right ascension, and the adoption of different, and therefore, at times, erroneous, values of the ratio of a "mean tropical year" to a "mean sidereal day," to transfer "mean solar times" into "sidereal times," and *vice versa*, which has led to error.

The subject is fully discussed in my paper in the *Monthly Notices*, 1893 December, and need not be repeated here.

But there is one point to which it is perhaps desirable that attention should be called, from the stress which Professor Newcomb lays upon his "table-of-logarithms" argument.

If quantities  $\theta^s$  are computed from a definite function  $f(t)$  of any variable  $t$ , in which the "angles" have been expressed in "time" at the rate of 86,400 seconds to an angle  $2\pi$ , the quantities  $\theta^s$  thus obtained for assigned values of the variable  $t$  will be identical whenever computed, and if we call the unit in terms of which  $t$  is expressed a "mean solar day," the quantities  $\theta^s$  computed from the equation

$$\theta^s = f(t)$$

will be identically the same for the same "mean solar time"; but the physical meaning of the variable  $t$  is not fixed by any such nomenclature. And although the numerical results will be the same whether the computations were made thirty years ago, are made now, or shall be made thirty years hence, it is *not* true that the *interval of time* which is called a "mean solar day" in practical astronomical work is independent of the conditions between the variable  $t$  and the facts of observation by which it is rendered determinate; it is not true that it cannot be changed by alterations of these conditions; it is not true that different errors in our time-determinations will not result from the introduction of different inconsistent conditions between the variable  $t$  and the facts of observation from which

our times are found ; and it is not true that the "mean solar day" as thus fixed will be independent of *any use which we may make of the computed quantities  $\theta^s$  during definite periods* in the actual determination of our "times" from observation. But these assumptions appear to me essential to give any force whatever to such arguments as those brought forward by Professor Newcomb against my views. But instead of these assumptions being true, the length of the *interval of time* which is taken as the "unit day," and called a "mean solar day," in such cases will be absolutely fixed by the conditions introduced between the facts of observation and the variable  $t$  by the particular results of observation with which we identify the computed quantities  $\theta^s$  to render  $t$  determinate ; and the times will be deducible from the equations of condition thus established, either exactly or only approximatively, according as the secular and periodic variations from the mean motion are perfectly or imperfectly allowed for in our theoretical investigations.

In practice we take the quantities  $\theta^s$  computed from a definite formula,

$$\theta^s = f(t),$$

or extracted from a selected set of solar tables, and use them as the "local sidereal times at the meridian transits of the Sun" for the corresponding "mean solar times" ; and we find our "local sidereal times" from observation, and therefore the "right ascensions of the Sun from observation," subject to the conditional relation between the local sidereal times and the variable  $t$ , or the mean solar times, thus established.

The use of a set of solar tables in this way for the determination of the local sidereal times *absolutely fixes the particular interval of time which is adopted as the "mean solar day" so long as the same set of solar tables is continuously used*. I have called the "mean solar day" thus fixed a "unit day" to distinguish it from the "physical" or "true" "mean solar day," an interval of time which we cannot change, although we can adopt different units in terms of which to express its length and that of any other intervals of time with which we may be concerned. The "unit day" thus fixed by the adoption and use of a set of solar tables in the construction of which an angle  $N$  has been used for the Sun's mean motion in longitude in a "unit day" will contain

$$86400 \left( \frac{N}{N_0} + \frac{N}{2\pi} \right) \text{ sidereal seconds ;}$$

where  $N_0$  denotes the Sun's mean motion in the "physical mean solar day."

The errors made by replacing  $N_0$  by the value  $N$ , adopted by Carlini, and the value  $N + \delta N$ , adopted by Le Verrier, cannot both be insensible. And the differences between the terms which

astronomers have neglected, before and after 1864, are such that they account for the *per saltum* change in the errors of Hansen's lunar tables as thus erroneously compared with observation.

If the "mean solar times" were always directly found, as they might be, subject only to errors of observation and of computation of the "equation of time," from the equation

$$\text{Local sidereal times at meridian transits of Sun} = f(t),$$

the change of the scale of time-measurement with the adoption of different solar tables in 1864 would have been directly apparent. To assume otherwise would be to assume that changes in the solar tables adopted for use lead to different "local sidereal times" for the "meridian transits of the Sun." But the change of time-scale is no less real because it has been generally overlooked, and if, whilst finding our local sidereal times, subject to the condition

$$\text{Local sidereal times at meridian transits of Sun} = f(t),$$

we introduce conditional relations between the variable  $t$  and the local sidereal times which are inconsistent with this condition, we cannot escape the consequences of these errors on our time-determinations; and it is these consequences which I have traced.

If Professor Newcomb is prepared to deny that we do *use a set of solar tables to render our local sidereal times observationally determinable*, it is desirable that he should state this. We should in this case be at issue about a matter of fact. But if such is not the case, he must *prove* and not assume that my mathematical deductions are erroneous; or my results should be accepted.

*On some possible Improvements in Meridian and Extra-Meridian Observing.* By Professor H. H. Turner, M.A., B.Sc.

1. The very interesting and important Paper by Dr. Gill, printed in the last number of the *Monthly Notices*, will at least have set many astronomers a-thinking.

The final answers to the three questions with which he concludes his paper cannot yet be given; but he is undeniably in the right in calling for a reconsideration of our astronomical methods—for a report by astronomers upon the organisation of exact astronomy. It is yet to be determined whether an International Congress or Congresses can be assembled successfully to this end. But if this project falls through, there is no reason why an effort should not be made at the present time to set down definitely in print floating ideas on the subjects to which Dr. Gill has referred in his paper; and if happily the

congresses meet, it will certainly facilitate their labours to have before them in type any definite proposals which may crystallize from discussion.

For this reason I venture to set down one or two small matters which have recently occupied my thoughts at intervals, in the hope that, if valueless in themselves, they may perhaps provoke criticism, comment, or digression which may serve a useful purpose. The theoretical advantages of any new method are apt to shine more brightly before practical trial than after, and are well known to be rapidly dimmed in lustre by the inevitable practical defects which experience reveals. I am therefore conscious that an apology is needed for thus calling attention to methods which are as yet untried; but I hope that Dr. Gill's Paper provides me with something like an excuse.

2. There are probably few who have worked with the transit-circle who have not also speculated on the possibility of avoiding some of its sources of error. Suppose we replace the telescope by a plane mirror, normal to the line of collimation, which is mounted between two carefully made pivots, and can be rotated about a horizontal axis in a manner precisely similar to a transit. And let us observe the image of a star by reflection from this mirror through a telescope *fixed* in the meridian at an arbitrary zenith distance, and pointing to the centre of the mirror; setting the mirror to the position appropriate to the N.P.D. of the star observed. It would seem that by an arrangement of this kind we should secure the following advantages over the simple transit-circle:—

(a) The observer might be under cover in a warm room and in comparative comfort; which is a condition of great importance in observing.

(b) His position would be the same for all stars; which would eliminate a considerable element of variability in his personal equation.

(c) In zenith distance observations many of the refraction difficulties would be avoided. The rays of light from a star to the mirror would not encounter any of the heated strata of air which necessarily occur in a transit-circle room, for the mirror could be in the open. In passing from the mirror to the observer such strata would be encountered, but they would be the same for all stars.

(d) The effect of flexure in a reflecting surface is much greater than (say) in an object-glass. But by making the mirror thick enough relatively to its aperture flexure may be reduced to insensible dimensions, and the instrument would then compare favourably with the ordinary transit-circle with its long tube, which is very liable to flexure.

(e) To develop advantage (a) to its full extent, the observer must be able to make all the necessary observations without moving from his seat at the telescope, including circle readings, and readings of the barometer and thermometers. There would



not seem to be any serious practical difficulty in arranging this, much in the same way as the numerous readings are made from the eye-end of a modern equatorial.

(f) The angle of incidence of the light of a star on the mirror is only small for a limited range in zenith distance so long as we use the same fixed telescope. But several fixed telescopes at different zenith distances might be used, and the results would very usefully check one another. Or the same telescope might be fixed in different positions from time to time, without disturbing the adjustments of the movable parts of the instrument—viz. the revolving mirror.

3. With the slight addition of a fixed telescope *not* in the meridian the *same mirror* can be converted into an extra-meridional instrument. Such a telescope will command, as the mirror revolves, a small circle of the sky whose pole is the east or west point, and whose angular radius is the inclination of the telescope to the west or east direction. The errors of adjustment of the revolving mirror may be determined by the meridian observations, the fixed telescope only introducing two additional constants—say its altitude and azimuth. This combination of telescope and mirror corresponds to a transit-circle whose telescope is inclined to the horizontal axis, and describes a cone instead of the meridian. The hour angle at which a body crosses the small circle commanded by such an instrument is independent of the latitude, and depends only on the declination of the object and the inclination of the telescope to the horizontal axis. The latitude of course affects the circle reading at transit; but there is an obvious advantage in not having the latitude enter into both co-ordinates, as in the case of the altazimuth.

4. Once the revolving mirror is set up, it may in fact be used to reflect into a regular battery of fixed telescopes. The project sounds an extravagant one, but these telescopes need not be large—say, 4 inches aperture—and much may be done with a single telescope placed in various positions if its co-ordinates be carefully determined on each occasion of shifting. A pair of telescopes, one of which was left untouched during the removal of the other, would give a virtually continuous series of observations.

5. Circles are now so admirably graduated that perhaps the circle is the part of a meridian instrument least in need of improvement. But the labour of determining division errors, and the risk of systematic error even after a careful determination, are responsible for the following crude suggestion. Returning to the mirror of paragraph 2, suppose a hoop whose centre is on the horizontal axis to be rotated by clock-work, and a mark on its inner surface to be reflected by the mirror into the field of view of a small telescope, fixed near the transit telescope; the rotation of the hoop would cause the image of the mark gradually to cross the field of view and disappear. But a second mark might be added which would appear as the first disappeared; and a

third again, and so on ; so that, in whatever position the mirror was placed, some mark on the hoop (which could be otherwise identified) would be crossing the field of view of the small telescope. Then the position of the mirror would be determined by the time of transit of the particular mark. The intervals of marks round the hoop would be determined by transits at leisure. Some accidental errors would doubtless be introduced by this method ; but the great safeguard against systematic error would be that the zero point would be different on each night, depending simply on the moment of starting the clock-work which rotates the hoop.

6. Concerning the clock, which would become of vital importance in both co-ordinates, if any idea such as that in paragraph 5 were carried into practice, the time would seem to have arrived when observatories of the first class should have not one standard clock, but a number, all kept at a nearly constant temperature, and isolated as far as possible from each other. Here, again, it is far easier to make a suggestion than find the necessary funds to carry it out. The idea of Professor Mendenhall and others, that a free pendulum should be used as an ultimate standard of reference as far as possible, is also well worthy of consideration.

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*The Effect of Personality in Observations of the Sun's Right Ascension on the Determination of the Position of the Ecliptic.*  
By F. W. Dyson and W. G. Thackeray.

(Communicated by the Astronomer Royal.)

In the introduction to the Five-Year Greenwich Catalogue of Fundamental Stars, shortly to be published, it is shown that the observations of the Sun for the years 1887-1891 indicate a correction of  $+^s.043$  to the adopted right ascensions of the clock-stars used in those years. It was considered advisable not to apply so large a correction to the right ascensions, and the Five-Year Catalogue is therefore referred to the epoch of the standard right ascensions of the Ten-Year Catalogue of 1880. Since then it appears, from the discussion of the position of the ecliptic for 1892 and 1893, that this large correction is more than confirmed, and if no cause can be assigned for it, then, following the precedent of previous years, a large correction to the right ascensions of all stars will be required in the next Greenwich Catalogue.

As the instrument, tables, and method of reduction are the same, and the only element of change in connection with the observations is the observing staff, it seems reasonable to infer a probable cause in the change in the *personnel* of the observers. It is well known that observers have large personal equations in measuring both the horizontal and vertical diameters of the Sun with the